ART. II.—*On Respiration.*—By JOHN W. DRAPER, M. D., Professor of Chemistry and Physiology in the University of New York.

I DESIGN in this paper to show that the explanation of the function of respiration, as usually given by physiological authors, is, in a very important particular, insufficient.

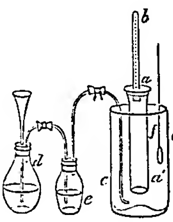
That explanation assumes that the passage of air to the blood presents three stages: 1st, its introduction to the trachea and bronchial tubes through atmospheric pressure; 2d, its further conveyance to the remotest air-cells by gaseous diffusion; 3d, its final access to the blood through the walls of the pulmonary vesicles and sides of the ramifications of the pulmonary artery by diffusion through those membranes. Thus, by the contraction of the diaphragm and other respiratory muscles, the capacity of the chest becoming greater, the air flows in from the external atmosphere, about seventeen cubic inches being introduced at each inspiration under average circumstances, a quantity scarcely more than sufficient to fill the nasal passages, the trachea, and the larger ramifications of the bronchial tubes; a portion of this air is immediately transferred to the cells by diffusive exchange, its oxygen passing instantly on to them, and carbonic acid and water coming in the opposite way; and, with a like rapidity, it percolates through the intervening tissues, and is commingled with the blood.

Now it is plain that this explanation necessarily involves the idea that the transit of a portion of air from the external atmosphere to the pulmonary vesicles takes place with very great rapidity; for, if it were not so, the quantity newly inspired, being nearest to the outlet, would be the first to be expelled by expiration. Under such circumstances, supposing breathing to go on, it would amount to little more than an alternate introduction and expulsion of atmospheric air, the effects not being felt at any great distance from the external orifice, and probably not reaching beyond the larger ramifications of the bronchial tubes. As the time elapsing from the commencement of an inspiration to the close of an expiration is only three seconds and a half, whatever changes occur in the constitution of this newly-introduced air must be accomplished with very great rapidity.

It becomes, therefore, a question for determination by experiment whether gaseous diffusion takes place with sufficient velocity to meet the conditions of the function of respiration. Although numerous experiments have been made on the *relative* velocity with which gases will diffuse into one another, I am not aware that any exact determination of the *absolute* velocity has been given. It has been taken for granted that it is very great, and some have even supposed that "a small quantity of carbonic acid would diffuse itself through a large volume of atmospheric air with the same rapidity that it

would dilate into a vacuum of the same dimensions." This is, however, altogether a misconception.

The arrangement I have adopted for the practical solution of this question is very simple. It consists of a tube four and a half inches long, and six-tenths of an inch in diameter, *a a'*, open at the lower end, the upper end being closed by a narrow tube, *a b*, divided into 100 parts, the joint at *a* being made air-tight by a cork. This compound tube, being full of atmospheric air, is suspended in a jar, *c c*, kept full of carbonic acid gas, which continually enters it from a generating apparatus, *d*, being the bottle containing the carbonate of lime and hydrochloric acid; and *e* a small vial containing water for the purpose of washing the gas and supplying it with watery vapour. The divided tube, *a b*, enters into the wide tube, *a a'*, by means of its cork, half an inch, so that the working length of the wide tube, from *a* to *a'*, is just four inches, and it is through that length that the diffusion takes place. The experiment is conducted as follows:—



At a given temperature, determined by the thermometer, *f*, the jar, *c c*, being filled with moist carbonic acid, the tube, *a a'*, containing atmospheric air, and having its graduated tube, *a b*, affixed to it by the cork, is suspended in the jar, as shown in the figure, for a given period of time. Diffusion of the carbonic acid now takes place in the upward direction, from *a'* to *a*, and, as soon as the given time has elapsed, the graduated tube, *a b*, is withdrawn from the cork, its extremity being instantly closed by the finger. The quantity of carbonic acid it contains is then determined by the introduction of potash-water in the usual way.

Now, it is clear that no carbonic acid can find its way into the tube *a b*, except after having diffused itself through the air in the wide tube *a a'*; that is, through a distance of four inches.

The experiments were made in a small room which was warmed by a stove, so that there was no difficulty in bringing the temperature to any required point, and, by a little management, keeping it stationary.

Experiment I.—The temperature being 60° Fahr., and time of exposure one minute, no trace of carbonic acid could be detected in the tube *a b*. It may, therefore, be inferred that that period is too short for diffusion to take place from *a'* to *a*.

Exp. II.—Temperature 60° F., and time five minutes; carbonic acid was found in the tube *a b*, potash-water removing seven parts therefrom.

Exp. III.—Temperature 75° F., and time five minutes; potash-water removes seven parts.

Exp. IV.—Temperature 90° F., and time five minutes; potash-water removes seven parts.

Exp. V.—Temperature 100° F., and time five minutes; potash-water removes eight parts.

From this, it would appear that, as the temperature rises, there is a slight increase in the rapidity of diffusion.

It was further found, on the closing of the wide tube at a' with a piece of tissue-paper, that the time of diffusion was greatly increased, being now twice as long as before. If the tissue-paper was wetted so as to be air-tight for moderate pressures, as shown by its becoming concave or convex by variations of temperature, the obstruction it occasioned was so great that in ten minutes no trace of carbonic acid could be detected in the narrow graduated tube. But when the tissue-paper was dry and perforated with pinholes, the time was shortened.

On examining these facts, it seems that the diffusion of carbonic acid into air, even where there is no obstruction and the distance to be traversed only a few inches, occupies quite a considerable period of time—a period which, of course, is increased if there be any obstruction or resistance in the way, and which must, therefore, be very greatly prolonged when the diffusion has to be made through slender, long, and winding passages.

The function of respiration could not by any possibility be accomplished in man if the interchange of gas contained in the pulmonary vesicles, with that most recently introduced, depended on molecular diffusion. The time requisite for such an effect to be completed through the narrow and long passages is far too great.

To some other agency we must, therefore, look for the complete explanation, and this, I think, is presented in the circular organic fibres of the bronchial tubes and cells. It has long been understood that these possess the power of varying the capacity of the tubes, and in morbid conditions, such as in the paroxysms of spasmodic asthma, their calibre is supposed to be very greatly reduced.

With this agency in view, we should therefore explain the respiratory act as follows: The carbonic acid, vapour of water, and excess of nitrogen, if any, which have accumulated in the pulmonary vesicles belonging to any given bronchial tree, are expelled therefrom by the muscular contraction of the circular organic fibres, and are delivered into the larger bronchial tubes, in which diffusion at once takes place with the air just introduced. As soon as the expulsion is perfect, relaxation of the muscular fibres occurs, and the passages and cells dilating both through their own elasticity and the exhaustive effect arising from the simultaneous contraction of other bronchial trees, fresh air is drawn into them; the alternate expulsion and introduction being accomplished by muscular contraction and elasticity.

I suppose that different bronchial trees submit to this action at successive periods of time, some being contracting whilst others are dilating.

In thus presenting the organic muscular fibres as the chief agent for the introduction of air to the blood, we raise them from the doubtful position they

have hitherto occupied in the estimation of physiological writers. "It is not known under what circumstances the contractile power which the bronchial tubes and, perhaps, the air-cells possess, by means of their organic muscular fibres, is brought into action. It is possible it may assist in expiration, but there is no evidence of its doing so."* From the foregoing experiments, it appears that the function of respiration cannot be explained without appealing to their agency.

The muscular action here contemplated produces a movement affecting the expulsion of foul air, analogous to the motion of food along the œsophagus, or of digested material along the intestines. In all these cases, the mechanical operation is the same. It is interesting to remark that in all instances the lungs are developed from some portion of the alimentary canal. In man, they appear as diverticula from the œsophagus, and, in much lower animals, from other parts of that canal. Thus, "in the Holothuriada, a membranous sac commencing near the cloaca and extending to the mouth, which, in the higher forms, is double, ramifies into respiratory trees, from which the expulsion of the water which has been breathed is effected by the contraction of the circular muscular fibres periodically. In the Bryozoa, the pharynx is dilated with the water, which, after aëration has taken place, is expelled. In the Tunicata, the pharynx, under the designation of the bronchial sac, becomes the special respiratory organ. In the Ascidians, the bronchial sac is emptied by the sudden contraction of its muscular walls. In the Salpians, the respiratory current is maintained by rhythmical contractions and relaxations of the muscular sac. In the Cephalopodous Mollusks, the current is sustained entirely by the muscular movements of the respiratory cavity."†

We might thus pursue the subject, and show that, in aquatic respiration, muscular contraction is constantly resorted to for the purpose of effecting the removal of the respired fluid. The same, too, is observed in the case of atmospheric respiration. It seems, indeed, to be the chief agent.

Concurring with muscular action is another force already recognized by many physiologists as taking part in the result. This is the motion of the vibratile cilia, which are found on the mucous lining of the bronchial tubes. In the lower orders, ciliary action is usually resorted to for ensuring a presentation of fresh portions of fluid to the respiratory surface.

The air, having arrived in the cells of the lungs, is next passed to the blood. In the lower tribes which have no disks, its oxygen is dissolved in the plasma; in the higher, it is taken by the hematine.

But before it can thus reach the disks, it must pass through the parietes of the cells, and also of the capillaries of the pulmonary artery.

Many physiologists have supposed that this exchange of oxygen for carbonic acid takes place on the principle of gaseous diffusion. On the authority of

* Kirkes and Paget's Physiol. Am. Ed., p. 131.

† Carpenter, General and Comp. Physiol., 1851, p. 740.

Valentine and Brunner, it has been asserted that the proportional exchange *actually* observed is 1174 of oxygen for 1000 of carbonic acid, these being the *theoretical* quantities under the law of diffusion.

Had the experiments, made twenty years ago in America by Professor J. K. Mitchell and others, been better understood in Europe, it would have been seen that this is a physical impossibility.

1st. In point of fact, the duty of the lungs is not restricted to the mere exchange of oxygen and carbonic acid, but is much more complicated. They likewise regulate the quantity of free nitrogen in the system. Oxygen, and very often nitrogen too, pass from the air to the blood; and there come in the opposite way carbonic acid, the vapour of water, and very often nitrogen. 2d. These various bodies are not all presented to one another in the gaseous state, as the theory would require, for some of them are held in liquid solution; and, were there no other reason, this alone, as Regnault and Reiset have remarked, is sufficient to show the inapplicability of that view. 3d. Even if all the exchanging bodies were in the gaseous state, the wall of the air-cell, of the pulmonary capillary, and of the blood-disk, would, by their condensing action, totally disturb the conditions.

With respect to this last point, I may refer to experiments which I published in this Journal fourteen years ago (*Am. Journal of Medical Science*, May 1838), from which it will be seen that the intervention of a pervious membrane of any kind totally alters the rate of exchange between diffusing gases. The law of Professor Graham is never mathematically true except when gases commingle without any intervening obstacle. The stucco partitions he used always disturbed the correctness of his results, as he found from his experiments.

If any one wishes to verify the force of this remark, it is sufficient to expose a soap-bubble, filled with carbonic acid, to the atmospheric air. If through its substance, which may be so thin as to be unable to reflect light, and, therefore, assume a velvet-black aspect, the gases exchanged under the law of Prof. Graham, for every 1000 parts of carbonic acid escaping 1174 parts of oxygen should be introduced, the bubble should consequently increase in size. But in a few moments it will be found to be collapsing, the carbonic acid escaping many times more rapidly than the air enters.

Under such circumstances, an aqueous film, not more than three-eighths of a millionth of an inch in thickness, can wholly disturb the law of diffusion, by the condensing action it exerts upon the carbonic acid and air respectively. What, then, should be expected from the moist walls of the air-cells and pulmonary artery, which conjointly are more than a thousand times as thick?

From these considerations, it would appear impossible to assign any definite rate as expressing the gaseous exchange between the interior of the cells and the blood; for, so far from being a case of exchange between two gases without any obstruction, the condition to which the law of diffusion applies, the nitrogen is, doubtless, in a state of solution in the blood, the steam in the

liquid condition of water, and, respecting the carbonic acid, nothing certain is known, whether it be in solution or chemically combined.

But while, for these pressing reasons, we are constrained to deny that the entrance of oxygen and escape of carbonic acid take place under the law of diffusion, and that their quantities are regulated by that law, there can be no kind of doubt that the phenomenon is wholly of a physical kind, in which substances in the gaseous state exchange with others dissolved in a liquid, or more intimately combined; the final rate being controlled by the condensing action of all the intervening membranes, the wall of the air-cell, that of the pulmonary capillary, and also that of the blood-disk. We have seen how a thin film of water can disturb these results, and no matter how thin the wall of the disk may be, it exerts its proper action.

It is generally admitted that the change of colour attending the arterialization of the blood is not so much dependent on the reception of oxygen and loss of carbonic acid, as on a change in the form of the disk, which becomes flatter when arterial, and more spherical when venous. The manner in which oxygen is associated with the colouring matter, the hematine, is perhaps not unlike what we observe in the case of another nitrogenized colouring principle, indigo, which, as is well known, can be oxidized and deoxidized, or turned white and blue, many times in succession, according as it is submitted to the action of atmospheric air, or to deoxidizing bodies.

If the foregoing observations are correct, we might explain the function of respiration in man, as follows:—

The air introduced by atmospheric pressure, brought into play by the action of the diaphragm and other respiratory muscles, fills, in ordinary respiration, the nasal passages, trachea, and larger ramifications of the bronchial tubes. Between it and the gas coming from the pulmonary vesicles, diffusion steadily takes place, tending to remove the cell-gas into the atmosphere; but this gas is not brought from the vesicles by diffusion, which could not act with sufficient speed, but by the contraction of the circular organic muscles of the bronchial tubelets and of the cells, the different bronchial trees not acting simultaneously, but successively. As soon as the contraction is over, the tubes expand by their elasticity, and air is drawn into the cells. It is probable that, in producing these results, the vibratile cilia conspire, and the effect is aided by the contemporaneous contraction of other bronchial trees, and the whole process ends with the expulsion of the foul air, which has accumulated in the larger bronchi and trachea by the diminution which ensues in the general capacity of the chest during expiration. In respiration, the lungs are not, therefore, passive, as is commonly said.

The exchange between the gas in the cells and that in the blood does not occur through simple diffusion, or in quantities proportional to the diffusion volumes of the oxygen and carbonic acid. It is a complex diffusion, in which the disturbances arise from the gases in the blood being either dissolved or combined; and through three intervening membranes, that of the air-cells, of

the pulmonary capillary, and of the blood-disk, all of which exert a condensing action, of the result of which it is impossible to furnish any numerical estimate.

Brought into presence of the hæmatin, the oxygen may possibly associate itself therewith, in a manner analogous to that which we witness, under similar circumstances, with deoxidized indigo.

In thus attempting to correct the account ordinarily given of the function of respiration, the only original points I present are, 1st, the necessity of admitting the *constant action* of the circular organic muscles; 2d, the *condensing action* of the three tissues, the wall of the pulmonary vesicle, of the pulmonary capillary, and of the blood-disk; 3d, the probable analogy between the relation of hematine, and another nitrogenized colouring principle, indigo.

ART. III.—*On Pleuritic Effusions, and the Necessity of Paracentesis for their Removal.* By HENRY I. BOWDITCH, M. D., one of the Physicians of the Massachusetts General Hospital. (Read before the Boston Society for Medical Observation, Oct. 20, 1851.) [With five wood-cuts.]

I HAVE chosen the above title to this memoir, in order that two distinct ideas may be brought before the mind, viz.: 1st, the simple idea of effusion of a fluid into the cavity of the pleura without reference to the character of the liquid, the cause of the effusion, or the length of time it has existed; and 2d, that paracentesis can be performed with safety, and should be done much more frequently than it is at present. I think that, in the course of this communication, I shall be able to convince the reader that individuals have died who might have been relieved by an operation; that paracentesis, as performed now by some Europeans, and in some parts of America, may be done with, at least, partial success in most cases, and with complete success and cure of the patient in others. Finally, I shall bring forward a method which, though originally suggested in Europe, was improved, and, so far as I know, first used in this country by Dr. Morrill Wyman, of Cambridge, Mass., a method more simple, less dangerous, and, in most cases, quite as effectual as the method pursued elsewhere.

From the times of Hippocrates, paracentesis thoracis has been at times performed, but generally as a last resource, and with a belief in the great danger attendant thereupon. Before Laennec's discovery of auscultation, it was impossible to arrive at a sufficiently accurate diagnosis at an early period of the disease; and, when some diagnosis had been made, the operation, as then performed, was much more dangerous than that which will be advocated in this paper. Laennec advises that the operation should be performed where dyspnœa, threatening life, supervenes in an acute pleuritic attack, and also in